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### Why things are different

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## 10. Why Things are Different

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The motivation for this book is the view that a lack of communication between scientists and policy-makers hurts both science and policy. On the one hand, there is the risk that economic theory becomes detached from the real world and hence becomes irrelevant. On the other hand, there is the danger that policy practitioners fall back on pre-scientific ideas or ideologies if they are not confronted with scientific insights. In short, lack of communication may produce disinterested scientists and ignorant policy-makers. Accordingly, we asked ourselves and the contributors the following questions:

- To what extent is communication between academic economists and policy practitioners lacking?
- Does the missing market for ideas have adverse consequences?
- If so, what inhibits the free flow of ideas and what measures should be taken to remove the barriers to trade?

As is common in economics, the answers are not straightforward; rather they are of the type 'on the one hand ..., but on the other hand ...'. One important reason why the conclusion cannot be simple is that the market for economists is large with a lot of variety. It does not make sense to throw all of economics on one heap. On the contrary, as Adam Smith pointed out, specialism pays. So diversity is, or can be, beneficial and economists should not be forced into the same mould. This chapter aims to bring out several dimensions of the problem, showing that while some arguments may be relevant for a certain type of economics, they need not be relevant for all economics.

### 10.1 Different Types of Irrelevance

There is a general impression with the public and even among economists that economic science does not offer much help in

addressing the major economic problems of today. There is a perception of mismatch between the supply of new findings by scientific research and the demand for useful knowledge by society. Malinvaud, in his contribution, reminds us that such concerns are not unique to economics. In any science, one may hear the complaint that research activity is too far removed from practical concerns. Should we conclude that science is irrelevant? The answer to this question is a simple 'No'. It is a misunderstanding that a science can be judged by its ability to predict or to solve problems, certainly when problems are complex. Scientific activity is the systematic attempt to understand the world and a science should be judged by the insights that it yields. Of course, economics is especially vulnerable to the above criticism since the failures of economists are so clearly visible. Economic problems are perceived to be very important in modern society. Economists thus have ample opportunities to show how little they understand of how the economy works.

An important question is thus to what extent the irrelevance of economic science is only perceived and to what extent it is real. If it exists, what causes it and what can be done about it? Portes argues that the irrelevance is only perceived. Economic science yields important insights and provides answers to those questions for which answers can reasonably be expected. Portes argues that the problems posed to economists are very complex ones to which an immediate answer cannot always be provided. If an answer can be given, the answer is typically not simple. Policy-makers and the public at large, however, desire simple answers. The simplification of the original answers creates disagreements among economists: some economists stress one aspect, others another so that trade-offs vanish into the background. Portes is not concerned about the failing predictions of economists. Making predictions is not the core business of economists. Moreover, predictions may be wrong simply because the input was wrong or incomplete.

Jacquemin, discussing the relation between the theory of industrial organization and competition policy, illustrates the important point that scientific progress, leading to better insight, need not make life simpler for the policy-maker. For example, when science uncovers new trade-offs, the policy-maker may be tempted to consider the new insights to be irrelevant. The 'old' industrial organization literature

was simple: theory postulated a linear relation between market structure, market conduct and market performance, while empirical work sought to relate performance and structure directly to each other. This simple framework turned out to be unsatisfactory: the empirical relations were weak and not robust and, theoretically, the linear relation was unjustified. This unsatisfactory state of affairs gave rise to a more sophisticated approach using game theory. It yielded a proliferation of models that all attempt to capture specific aspects of the complex interrelationships between structure, conduct and performance. One may regret the resulting fragmentation, but the proliferation of models may simply reflect the fact that the world is more complicated than was initially believed. Paradoxically,

we may conclude that the models produced by the new Industrial Organization in this domain have improved the quality and the relevance of our analysis. Nevertheless, simultaneously, they have made the dilemma faced by the Antitrust Authorities more complex.

Schelling discusses three reasons for the irrelevance of economics, which all originate from the fact that many policy discussions are dominated by value judgments to which economists have little to contribute. The most extreme form that Schelling discusses is when the policy debate remains entirely ideological, when the decision makers (and/or the public) refuse to discuss the issue in a rational, scientific way. People may hold such strong convictions or beliefs that they may be unwilling to be persuaded by a scientific analysis. As Schelling remarks, it may be a rational strategy for a government official seeking reelection not to put a value-laden item on the agenda: 'The issue ... cannot even be raised in public by a senior government official without a resulting clamor for resignation'.

A second reason why economists may have little to contribute to the policy debate is that the economic arguments may simply be banned from the discussion. Schelling sees two main contributions of economic analysis to environmental policy: cost-benefit analysis and replacing direct regulation by market incentives. Yet

environmental regulation ... is, or is perceived to be, about saving lives. Life and death are a subject that many people, including legislators and administrators, believe should not be contaminated by cost considerations.

A third reason is that economists themselves may be divided about their value judgments. Recall the distinction between positive economics (that aims at understanding and is essentially value free) and normative economics (that aims at providing policy advice and in which value judgments enter). According to Friedman

differences about economic policy among disinterested citizens derive predominantly from different predictions about the economic consequences of taking action - differences that in principle can be eliminated by the progress of positive economics - rather than from fundamental differences in basic values ... about which men can ultimately only fight. (Friedman 1953, p. 5)

Friedman is thus optimistic that progress in economic science will lead to agreement on policy. Schelling's contribution, however, suggests that essential differences will remain, that even if understanding is increased, economic insights will not settle differences of opinion because they will not close the value gap.

This completes our overview of arguments why economics may not be as irrelevant as is sometimes thought and why economic science should not always be blamed for perceived irrelevance. This classification basically deals with the demand for economic knowledge. The next sections explore the supply side. Could economic science be even more relevant? If so, how could this be achieved?

## **10.2 Different Types of Economics**

The spectrum of research activities that economists perform ranges between two extremes, from fundamental research to policy research. In the interior of the spectrum falls 'applied' economics, which transforms the insights from fundamental research into principles that guide policy and applies the tools provided by a combination of science and statistics to address either problems uncovered by policy or puzzles turned up by the economic system. Boundaries are, of course, vague. Moreover, as Portes remarks, applied economics should not be equated with policy analysis: it is substantially broader. Portes also makes the value judgment that good economics involves both thorough knowledge of theory and sophisticated application.

If Portes' values were widely shared in the profession, praise should go to those who are able to successfully apply existing theory, not to those who develop theory, nor to those who implement the insights resulting from application. Malinvaud does not reveal his values but he suggests that, at present, researchers receive too little credit for good pieces of economic work:

the reward goes too much now to mathematical skill ... too much praise is given to building and solving models of disputable relevance; too little is given to good pieces of economics as long as they contain no mathematical model.

In short, the incentives are wrong; they yield research, that, as Frey and Eichenberger argue, focuses more on rigour and formal elegance than on providing insights on how the economic system actually functions. Furthermore, Frey and Eichenberger are concerned that increased globalized competition and intensified competition will further strengthen the incentives to perform irrelevant research.

The problem of the wrong balance in economic research can be approached from three alternative angles. First, consider the tools that a scientific economist has at his disposal. Schumpeter (1954) distinguishes four fundamental fields: history, statistics, theory and economic sociology. Using modern terminology, we would say that a professional economist should have a good knowledge of the relevant economic institutions, of the data of the economy (as well as of the statistical methods used to compile these and the problems involved), of the historical evolution of these data and institutions, and of economic theory. Whereas economic theory supplies the tools of analysis, the other three fundamental fields supply the content material to which the tools can be applied.

The imbalance of research amounts to a criticism that there is excessive emphasis on theoretical refinement at the expense of the other fundamental 'techniques'. Hence emphasis is on improving the methods of analysis, rather than on content. As Geelhoed writes, academic economists have to a large extent specialized in

highly mathematical analyses that excel in rigour but not in applicability to ... real world problems [and a] lack of a clear understanding of the institutional context and of the characteristics of the national economy is a deterioration.

Next, consider the activities of the scientific economist. In essence, scientific activity comprises three different stages:

- observation of the real world and construction of a simplified model that incorporates the essential elements of interest;
- deduction of the consequences of the model; and
- confrontation of these consequences with observed data.

Of these three stages, the second one is purely logical and mathematical. The first and the third stages - the creative construction of the model including the judgment involved in selecting the 'right' model - and the confrontation of the insights derived from the model with the real world, constitute the essence of economics. While the praise should go to those economists who excel at the first and/or third stages, it actually goes to those with mathematical skills. Consequently, too much emphasis is given to the second stage (the analysis of models of disputable relevance) and too little to the first and third stages.

Consider the markets on which the academic economist is active. The demand for his services arises from two sources: from his fellow scientists and from the policy-makers and the public who want to be enlightened on how the economic system functions. Critics argue that research efforts are too much directed to concerns that are purely internal to the science at the expense of efforts devoted to practical concerns. Economists are working on puzzles that they cooked up themselves, rather than on models and problems that are inspired by reality. Of course, such an argument applies to any science: as a science develops, it automatically generates intellectually challenging problems not directly related to practical issues and some scientists are motivated to try to solve these. Malinvaud argues that this may be beneficial in the long run. Nevertheless, he suggests that purely scholastic investigations may prosper for too long and that this problem may be especially serious in economics.

After having presented these different perspectives on the problem, we now turn to analysis. Why would economists specialize on the wrong tools and the wrong problems? Why would applied research be undervalued? Why are the incentives wrong?

Frey and Eichenberger locate the reason for the faulty incentives in the academic review process. They write:

The quality of a professional contribution can only be evaluated with respect to internationally valid aspects. Formal rigour and elegance perfectly meets this requirement[, but] academic contributions based on an extensive knowledge of local conditions and institutions cannot be judged by an external scholar.

Frey and Eichenberger argue that aspects that are difficult to judge will not be judged, especially not in a situation where there are multiple referees who are not familiar with institutional settings. Accordingly, only technical aspects - rigour and formal elegance - are evaluated. Obviously, if evaluators do not pay attention to a certain aspect, then, given the incentive to publish, the researcher rationally chooses not to devote much attention to it. Hence, the overemphasis on formality and theory. In Frey and Eichenberger's view, a scientist is driven both by an intrinsic motivation to understand the world and by the incentives that the academic market provides. The latter incentives are strongly biased against relevant real world issues. According to Frey and Eichenberger, globalization and increased competition worsen the situation: in an international market it is less likely to meet a referee with knowledge of local conditions. Hence, the relevance of the model is even more difficult to judge and thus gets correspondingly less weight.

Whereas these arguments probably contain some truth, we doubt that it is the entire truth. If correct, there should be even less empirical and institutional content in research papers. The constant flow of heavily empirically oriented NBER working papers cannot be considered as trivial examples of elegance of technique. Indeed, one cannot accuse the 'stars' of the profession (winners of Nobel Prizes, the Bates Clark medal, and so on) of doing irrelevant work. Some of the very best people in the profession are also involved in empirically oriented work, suggesting that such work does bring substantial professional payoff. In fact, van Dalen and Klammer show that both Americans and Dutch graduates considerably appreciate economists who do applied work that sheds light on the real world. Perhaps a science should be judged on the basis of the best work that it produces. One has to accept that a large proportion of the research is irrelevant, because this is inherent in the business. In short, it does



not seem to be the case that the profession does not value good applied work.

Given that empirical work is desirable and at the same time both labour and capital intensive, it seems worth the trouble to design new incentive structures that favour empirical work. European funding of research networks, teams of research workers that devote themselves to the same topic across Europe, could be an important stimulus in this respect. It is not only a matter of money. In particular peer-review systems can deliver more and better empirical studies only if referees take the importance of such work better into account.

### **10.3 Different Types of Theory**

Theory is not always interpreted in the broad sense that Malinvaud rightly argues should be the case:

Theory should ... be understood in the broad sense it should always have. A theory refers to a phenomenon or a problem and it provides a methodical intellectual construct, of a synthetic nature, for the knowledge and analysis of the phenomenon or problem.

The narrow meaning that equates theory to a mathematical model is used quite frequently. This may lead to sterile formal theorizing that could be rightly accused of being too far removed from the real world. At present, the two conceptions of theory exist side by side in economics. We labelled the extremes 'high tech economics' (that is, a sterile, formalistic non-contextual approach) and 'human capital economics' (high quality applied research that is transparently linked to the context of the real world).

Colander (1992) argues that economic theory should be allowed to stand by itself: one should not insist on direct policy relevance. If positive economics is freed from the constraint to be policy relevant, it might be more imaginative and ultimately more useful. Moreover, it frees applied economics from the perceived need to employ the methodology of positive economics. Solving problems is a different activity than testing theories. It involves judgment about which theory is most relevant and applying it while taking its limitations into account. A rigorous scientific procedure is not always necessary, nor desirable, to reach solutions to an actual problem. In the situation

where the solution depends on the integration of solutions to various sub-problems, the results of an analysis can only be as exact as the least precise part of the analysis. Accordingly, there is no point in improving the exactness of just one part of the solution if the other parts remain very imprecise. In particular, if the facts are difficult to ascertain, using sophisticated technical econometric analysis to analyse these data may not make much sense.

One reason for the advancement of 'high tech theory' may have been the combination of extensive specialisation (within academia there are experts in every technique) and a lack of communication between the various experts. Why has so little trade occurred between the specialists? Why did each cater only to his own specialized market? We discuss these questions in the next section. For now, we conclude that the different types of theory coexist, that there is room for both, but that one type may be more valuable than the other, and that the latter may be overrepresented at present.

#### **10.4 Different Types of Economists**

We have defined a professional economist as a person who masters certain 'techniques' (history, institutions, data, statistical methods, and theory) and who is supplying ideas and insights derived from these 'techniques' on various markets. We have distinguished between three markets: fundamental research, applied research and policy advice. In addition, there is some demand for general enlightenment on economic issues by the interested public. This is a fourth market on which economists might be active.

At the time of the classical economists, when economics had not yet established itself as a profession, an economist was a multi-market economist. Now that economics has turned into an industry, each economist specializes in a certain mix of techniques and typically focuses on a specific market. Van Dalen and Klammer distinguish between the academic professional (who aims at obtaining applause from his peers), the researcher (whose objective is to conduct research that is relevant for decision making in policy) and the policy adviser (who seeks to enlighten the policy process and who want his advice accepted). Each market requires its own mix of techniques; the criteria for success differ across markets. As

economists, we are well aware of the fact that it pays to specialize. Different markets may require different types of expertise to be successful. For the scientist who wants to extend the stock of knowledge, specialization in a narrow field may be essential. However, the ability to synthesize and generalize may be required to be a successful policy economist.

Of course, the ability to convince others is essential for all types of economists. We may agree with Theeuwes that an economist's objective is to convince others of the value of an idea or insight that he or she has developed. In the academic market one has to convince the editors and referees; in the policy market one has to convince users of the practicality of the idea. Theeuwes's contribution raises the question of whether different marketing techniques are appropriate in different markets. Theeuwes seems to focus on the market for policy advice. He suggests that the policy economist may present the evidence selectively, that is only the evidence that supports the case. He even suggests that an economist may at one time defend one position and at another time the opposite position. Yet, at the same time, Theeuwes insists that the economist be honest. But is the economist who presents selective evidence honest? Is presenting selective evidence allowed in the academic market? Malinvaud disagrees with Theeuwes: 'Economists ... should refrain from taking sides when conclusions are not clearly determined'. Hence, is it really appropriate to compare an economist to a lawyer or should the economist act as a neutral expert?

Not only should different types of economists exhibit different abilities, the applied economist should also master a different set of tools than the pure scientist. Practical economic problems cannot be solved by economic theory alone. It is inevitable that non-economic considerations enter as well. Pure scientific activity differs from problem solving; ultimately, the quality of a piece of applied work is judged by the quality of the solution for the problem that was posed.

Fundamental differences concerning quality and value added exist between the various markets. The scientist adds knowledge, he pushes the knowledge frontier outward. Here analytical capabilities are especially important. The engineer applies existing knowledge so that the ability to synthesize existing knowledge is important. It will be rare that the combination is present in one and the same person. In

addition, each market needs its own special expertise. For example, political talent is more important in the policy market than in the academic market. Furthermore, in the policy market one has to be quick, whereas endurance is important in the academic market. It will be very rare indeed for a person to be successful, or be able to compete, in both markets and good economists probably are the rarest of birds.

### **10.5 Different Types of Funding**

Of all contributors to this book, Frey and Eichenberger are the most sobering about the future of economics. They predict that increased global competition will strengthen the incentives to do irrelevant, formal research: economics departments will be transformed into small departments of applied mathematics. While most of their analysis is positive, based on the assumption that economists, and their work, are based on too narrow an objective standard (rigour and elegance), they suggest also that a more broadly based evaluation scheme (in which policy advice is explicitly taken into account) may offer some consolation.

We agree with Portes that users should not dictate the scientific research agenda. At the same time, we believe that we should think hard about new incentive structures that would tilt the balance in favour of applied work. Each scientist is confronted daily with many problems that are internal to the science, because other scientists are eager to communicate their results, which always throw up new problems. However, most academic economists are not in close contact with the practitioner so that they encounter his problems much less frequently. Many relevant real world problems are not communicated directly, but rather through noisy channels such as the media. Couple this observation with Solow's (1989, pp. 39-40) admission that most economists lack a talent for direct observation and it is clear that the probability that an academic economist picks up a relevant, tractable 'real world' problem is much lower than that he picks up a problem communicated by a fellow scientist. Economic science needs the help from practitioners to stay on the right track.

In this respect, intensive communication between academics and practitioners is of the utmost importance. Hence rotation between the

different types of jobs might be very beneficial. While users should not dictate the academic research agenda, they should inform scientists about their problems and needs. Of course, given that research takes time, the contacts are especially worthwhile if the information is provided by users with 'Vision', by those who can anticipate the future problems to a certain extent. Certainly, as Geelhoed remarks, policy-makers should not wait until academics have finished their job. In the discussion that followed after his talk, Geelhoed mentioned various European policy issues that he believed would figure prominently in the near future: the debate on location, the economics of public infrastructure, and the impact of EMU in a system of economies with widely differing institutional settings. Economic science should not follow but rather prime the policy debate. In response, Portes argued that curiosity driven research has yielded important insight on location problems and that a lot of research has been done on EMU, driven by the perception of economists that this an important problem. In Chapter 9, Geelhoed discusses the case of CEPR's timely research on the transition of Eastern Europe. Here Geelhoed and Portes agree on both the research agenda's topic and on the need for first class economic research. However, they disagree fundamentally about what is good research. The available research is not Geelhoed's cup of tea. What Portes considers 'first class' is irrelevant in the opinion of Geelhoed.

Instead of reducing the costs of doing relevant research, one may attempt to raise its benefits. In this connection a suggestion made in Tullock (1989) deserves to be taken up. Tullock starts from the observation, also mentioned in Portes' contribution, that the problem arises from the fact that economists generally do not produce exploitable innovations. Consequently, Tullock turns to systems with patentlike properties. He suggests organizing tournaments in order to reward research *ex post*. 'That is, instead of trying to guide the research in advance by research grant proposals, we simply pay people in terms of the potential merit of the research after the research is done. It is easier to judge the value of a research project after it is finished than before it is started' (Tullock 1989, p. 242). Of course, fundamental research is rewarded by a similar process (a publication is not guaranteed at the start). By rewarding also applied

research in this way, the quality and quantity of applied research could be raised.

Obviously, given its public good character, the government has an essential responsibility in funding fundamental, curiously-driven 'blue-sky' research. The trend observed by Portes that government agencies are reducing funds for this type of research and that they insist on societal relevance in addition to, or perhaps even as a substitute for, scientific quality, is somewhat worrying, but understandable. While Geelhoed disagrees with Portes on many aspects, he agrees with him on the value of 'blue-sky' research and he points to risk aversion on the part of funding institutions as a possible explanation why various fundamental questions seem to have received only very limited attention from economists. The Dutch funding agency asks research projects to be evaluated according to their feasibility (a referee has to judge whether one can have confidence that the project will be successfully completed, taking into account the reputation and quality of the proposer and his experience in the field). Obviously, there is nothing wrong with insisting on quality. However, the arguments from Frey and Eichenberger imply that insisting on experience inhibits scientific progress: it makes it more difficult to get funding for innovative research that falls between two rays. Colander (1992, p. 194) argues that current abstract thinking is application of technique to precisely defined problems and that such work seldom leads to significant advances in science. The method of funding, however, is biased in favour of this type of insignificant work.

### **10.6 Different Types of Teaching**

Academic economists have not only been criticized for the perceived lack of relevance of their research; they are also responsible for the training of the future generations of economists and their teaching has been criticized as well. Geelhoed complains that 'whereas 90 per cent of our students do not want to become scientists, both graduate and post graduate education still appear to aim at providing the foundation for a scientific career'. In Geelhoed's opinion, Dutch students are trained in techniques rather than in relevant applications or economic history. Moreover, whereas students are motivated by the problems of the Dutch society, they are trained to solve problems of the US

economy. In other words, education caters mainly to the academic market and educators seem most interested in training and selecting good future direct colleagues. Consequently, demand for other types of economists is not met very well so that new entrants into the Dutch administration have to be retrained.

In Malinvaud's view the main criticism is that teaching gives too much attention to abstract problems and too little attention to the most pressing problems of modern society. There is a 'mismatch between the supply of knowledge to students and the demand of society with respect to what students ought to know'. In his view two other supply-side problems are that teaching focuses on theory out of context (the emphasis is on the pure theory without discussing the empirical foundations so that students do not have a good idea about the domain of relevance of a theory) and that teaching concentrates too much on fads and fashions in economic research. A larger distance from current research is preferable since it gives more insight and reduces the need for formalism: teaching should concentrate on the ideas that have already proved themselves.

This mismatch originates in the incentives that university teachers face. Evaluation is based mainly on scientific achievement whereas teaching consumes research time. The 'time lost' is minimized by focusing teaching on issues that are as close to research as possible. Similarly, since academics profit directly from well trained colleagues, it is natural that they focus their effort on it, unless incentives are in place that induce them to do otherwise. Given that 90 per cent of the students finds a job outside of academia, it is surprising that such incentives apparently are either absent or weak. Either the retraining by employers that Geelhoed refers to is efficient or competition in the market for education is lacking.

One reason for insufficient competition might be a lack of information. Hence job mobility might help because economists who have worked on both sides might provide a clearer perspective on the necessary ingredients of education. Harberger (1993, p. 22) notes that economic education 'should import the type of simple and robust theoretical framework that economists will be able to use for the rest of their lives, and also how to use it'. Since applied economists have to be able to respond quickly, they have to be able to think on their feet. Therefore, they have to know simple tools and how to use them

well. Indeed, economic education should train students to 'think like an economist'.

Probably education currently focuses too much on introducing new tools without showing how these tools can be applied to real world issues. Economists seem to believe that knowledge of the tools automatically yields knowledge about when and how a tool can be used. Schumpeter already noted that this is a misunderstanding:

Everybody knows that in order to play chess it is not enough to know the figures and how they move. It should be equally clear that the mere knowledge of definitions and theorems is not enough to play the scientific game ... what one ought to learn is to work with such theories, how to analyze concrete situations and how to solve problems with them. (Schumpeter 1982, p. 1059 and p. 1055)

In Schumpeter's view, the training in the handling of economic tools (and in particular theories) should be the essential task of economic education. He believes much of teaching is deficient in this respect. Even among professional economists, 'thoroughly competent ones are comparatively rare ... current discussion of economic questions almost always displays the sad fact that some and occasionally all who take a hand in it do not know what they are talking about' (p. 1058). These discussions 'are all like duels between combatants who have not learned the art of fencing' (p. 1052).

The discussion above does not necessarily conflict with the view of Frey and Eichenberger that policy advice essentially requires core economics. However, it makes it doubtful whether the core can be taught in one semester. Perhaps the essence of the core (that is, rational behaviour and the resulting equilibrium) can be taught in just one semester, but by then one knows only the rules of the game, not how the game is played (or could be played) and what insights could be gained by playing it. One needs considerably more time to learn how to use these basic tools and how to use them well. After all, there is no demand for students that have attended courses for only one semester. Even when the useful economic principles are simple, it might take considerable training and experience in applying, before their strengths and limitations are sufficiently understood.

We agree with Schumpeter (1982, p. 1054) that 'it is no advantage for a science to be too easy, for this will deter good and



attract mediocre minds and create a strong party of opposition to achievement and refinement'. Indeed economics suffers from the fact that not everybody realizes that

economics is not a doctrine which one might accept or reject but rather an arsenal of theoretical tools which you have to train yourself to use before you can have any opinion about its usefulness or otherwise and that many of the shortcomings of the science are caused by the incompetence of very many economists, who never learned their own business and turn to politics and philosophy because they are not up to the task of the scientist (Schumpeter, 1982, p. 1053).

Economic education, in our view, should focus on the training of the use of the tools. At the same time it should convince students that these tools are powerful enough to enrich the understanding of the real world.

### **10.7 Different Types of Conclusions**

Different conclusions can be drawn from the material presented here. One obvious conclusion is that the (ir)relevance of economics is a difficult subject; that the problem, if there is one, may not have an obvious solution. The market for economic research may not work perfectly, but it is not crystal clear how it could be made to work better.

Portes and Frey and Eichenberger present the two extreme views in this book. Frey and Eichenberger argued that something is fundamentally wrong with our discipline and that something needs to be done about it. We argued that their analysis may be based on shaky foundations and that their policy recommendations need not lead to improvement. Portes, in contrast, believes that the market works perfectly and argues accordingly. We are not convinced by his arguments that the self-regulating mechanisms of the profession can be relied upon. Actually, it is somewhat strange that, at a time when economists (as 'guardians of sound principles') advocate deregulation of many sectors of the economy and thereby point out the dangers inherent in self-regulation, one distinguished economist essentially backs up his arguments by relying on the virtues of the self-regulatory mechanism for his own profession. The laws of economics

apply to economics as well. Hence, as economists we should accept that the satisfaction of demand is the acid test that we should meet. Accordingly, the fact that users complain should concern us.

Thusfar we have restricted ourselves to setting straight, to pointing out which problems are real and which ones result from misperceptions. We have argued that, when one takes a detailed look, many problems are only perceived. Hence, these problems could be resolved by improved communication between the various stakeholders in economics. Indeed, a plea for improved communication has been a thread running throughout this chapter.

A second thread has been that there is no point in lumping everything and everyone together: we need diversity! Policy engineering as well as the 'guarding of sound principles' can play a useful role in policy-making. Both applied research and purely speculative 'blue-sky' research serve an important purpose. The science of economics should be distinguished from its applications; if the engineering part, perhaps, is not very successful, the baby (the science) should not be thrown out with the bath water; science can stand by itself. Diffusion of knowledge is important, just as innovation is. Moreover, we should recognize the bounded and different capacities of individuals so that we can exploit differences in talent as much as possible. Different markets require different skills. Very few people are good at more than one thing. Hence specialization pays.

Our advice amounts to recognizing the advantages of specialization and to exploit these advantages as much as possible by allowing for free trade and competition in ideas. As economists, we believe that competition may improve efficiency. Indeed, we have not been fully convinced by Frey and Eichenberger that intensified competition has adverse effects, although we take their analysis as a serious warning that we should carefully analyse the structure of the market for economic ideas. Clearly, for trade to occur, communication is necessary and we suggest removing barriers to the exchange of ideas. By improving communication channels, the information base and the transparency of the markets may increase and this, in turn, may lead to intensified competition with positive effects.